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THE IMPACT OF THE POTENTIAL DURATION OF UNEMPLOYMENT BENEFITS ON THE DURATION OF UNEMPLOYMENT

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ABSTRACT

This paper uses two data sets to examine the impact of the potential duration of unemployment insurance (UI) benefits on the duration of unemployment and the time pattern of the escape rate from unemployment in the United States. The first part of the empirical work uses a large sample of household heads to examine differences in the unemployment spell distributions of UI recipients and nonrecipients. Sharp increases in the rate of escape from unemployment both through recalls and new job acceptances are apparent for UI recipients around the time when benefits are likely to lapse. The absence of such spikes in the escape rate from unemployment for nonrecipients strongly suggests that the potential duration of UI benefits affects firm recall policies and workers' willingness to start new jobs. The second part of our empirical work uses administrative data to examine the effects of the level and length of UI benefits on the escape rate from unemployment of UI recipients. The results indicate that a one week increase in potential benefit duration increases the average duration of the unemployment spells of UI recipients by 0.16 to 0.20 weeks. The estimates also imply that policies that extend the potential duration of benefits increase the mean duration of unemployment by substantially more than policies with the same predicted impact on the total UI budget that raise the level of benefits while holding potential duration constant.

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I. Introduction

European countries with relatively generous unemployment insurance (UI) systems (such as Belgium, France, Germany, the Netherlands, and the United Kingdom) have suffered much larger and more persistent increases in unemployment in the 1980's than the United States. These differences in European and U.S. unemployment experience are largely explained by the substantially longer duration of unemployment spells in Europe. Furthermore, much microeconomic evidence indicates that there is a positive relation between the level of UI benefits received and the duration of the unemployment spells of UI recipients.[1] These observations have generated much interest among both academics (e.g. Minford, 1985) and the press (e.g. The Economist, 14-20 May 1988, p.69) in the hypothesis that work disincentives arising from generous unemployment insurance (UI) systems play an important role in high and persistent European unemployment in the 1980's.

Burda (1988) provides some suggestive evidence that differences in the generosity of UI systems may help explain cross-country differences in unemployment performance. Burda finds a strong positive correlation (of 0.63) between a measure of the generosity of UI benefits available to a fully insured worker and the ratio of long-term unemployment to the labor force for fourteen OECD countries

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[1] See, for example, Classen (1977) and Solon (1985) for estimates of the impact of benefit levels on spell duration in the United States, and Atkinson et al. (1984) and Narendranathan et al. (1985) for estimates for the United Kingdom.

in 1985.[2] On the other hand, Burtless (1987) argues persuasively that European UI systems were generous well before the rise in European unemployment. Many European economies with very generous benefits had much lower unemployment than the less generous U.S. in the 1960's. Still, typical unemployment spell durations have tended to be quite long in economies with liberal UI systems even in periods of relatively low unemployment. A key issue prior to the debate over the impacts of UI system generosity on aggregate unemployment is whether there is microeconomic evidence consistent with the key link in the macroeconomic argument that observed differences in UI systems can help explain substantial cross-country differences in the duration of unemployment.

In comparing the UI system in the U.S. with those in Europe, the major differences appear to be in the potential duration of benefits and eligibility requirements for benefits rather than in the weekly benefits level for qualified workers. In particular, insurance and other assistance lasts for more than twice as long in most European countries than in the United States. The potential duration of benefits varies dramatically across countries. The typical qualified worker is eligible for 6 months of benefits in the U.S. versus over a

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[2] Burda classifies as long-term unemployed those unemployed workers with current spells of 12 months or longer. The UI measure constructed by Burda combines information on the level of benefits, average manufacturing earnings, and the maximum duration of benefits. Burda's measure is the present discounted value (using a 10 percent discount rate) of the maximum number of weeks of benefits available to an insured worker relative to the average weekly earnings for a manufacturing worker. This measure ignores taxes and substantial differences across countries in eligibility requirements for UI. These differences in eligibility rules generate substantial variation in the fraction of the unemployed covered by UI across countries.

year of benefits in the France, Germany, and Sweden (Burtless, 1987). In fact, benefits can last indefinitely (at a reduced rate after the first year) for some individuals in Belgium and the United Kingdom (Emerson, 1988). UI benefits that last for a long duration combined with limited monitoring of search effort may make an economy more susceptible to increases in long-term unemployment in the face of adverse shocks.

While much microeconomic research has shown that higher levels of benefits are associated with longer durations of unemployment, there is much less empirical research on the impacts of the potential duration of benefits on the duration of unemployment.[3] Since differences in the length of available benefits are the key difference among UI systems, an understanding of how potential benefit duration affects the distribution of unemployment spells is crucial for determining whether UI differences help explain cross-country differences in unemployment.

In this paper, we present new empirical evidence on the impact of the level and potential duration of benefits on the duration of unemployment in United States. Since the prospect of rehire by one's previous employer is important for a substantial fraction of UI recipients in the U.S.,[4] we examine the impact of UI benefits on

[3]Moffitt and Nicholson (1982), Moffitt (1985), and Ham and Rea (1987) are among the few sophisticated econometric studies of the impact of potential benefit duration on the duration of unemployment.

[4]For example, Katz and Meyer (1988) found that 75 percent of a large sample of UI recipients from Missouri and Pennsylvania in 1979-80 indicated when they filed for UI benefits that they expected to be recalled by their previous employer.

firm's recall polices as well as on worker new job acceptance behavior.

We look at two types of empirical evidence. The first part of our empirical work involves an analysis of the unemployment spells of a sample of household heads from the Panel Study of Income Dynamics. These data allow us to compare spell distributions for UI recipients and nonrecipients and to look at differences in the time pattern of recalls and the acceptances of new jobs. We find big differences in the distribution of spell durations for UI recipients and nonrecipients. Sharp increases in both the recall and new job finding rates are apparent at durations when benefits are likely to lapse for UI recipients. The absence of such increases in the escape rate from unemployment for nonrecipients provides strong evidence of an impact of the potential duration of UI benefits on firm recall policies and workers' willingness to start new jobs.

The second part of our empirical work examines the impact of the level and length of UI benefits on the escape rate from unemployment for a large sample of UI recipients. This Continuous Wage and Benefit History (CWBH) data set, extracted by Moffitt (1985a), has the advantage of providing detailed administrative records on the UI system parameters facing individuals. Since the data set covers spells in 12 states during the 1978-83 period, a fair amount of both cross-section and time series variation in UI parameters is available. This variation allows us to directly estimate impacts on the escape rate from unemployment of differences in the level and length of benefits and test the predictions of alternative models.

We utilize these estimates to simulate the impact of changes in the level and maximum duration of benefits on the mean duration of unemployment, the fraction of workers exhausting benefits, and expected expenditure on UI benefits per compensated unemployment spell.

The remainder of the paper is organized as follows. Section II reviews several alternative theoretical models of the effects of UI system parameters on the probability of leaving unemployment. Section III presents our comparison of the unemployment spells of UI recipients and nonrecipients. Section IV applies econometric duration models to administrative data on the spells of UI recipients, and section V presents simulations using these estimates. Section VI provides some concluding remarks.

II. Theoretical Background

In this section, we analyze the likely impacts of the level and potential duration of unemployment benefits on duration of unemployment and the time pattern of the escape rate from unemployment. We discuss the predictions of three types of models: (1) a standard job search model; (2) job search models that incorporate the layoff-rehire process; and (3) a static labor-leisure choice model.

Standard Job Search Model with no Recalls

Mortensen (1977) utilizes a dynamic search model with no recall possibility, variable search intensity, a stationary known wage offer distribution, and a constant arrival rate of job offers (for a given search intensity) to analyze the effects of UI on the escape rate from unemployment. Mortensen incorporates two key features of the UI system in the United States into the model: benefits are assumed to be paid only for a specified duration rather than in every period of an unemployment spell, and new entrants or workers who quit jobs are not qualified for benefits.[5]

As the remaining number of weeks of benefits available to a qualified unemployed worker decreases, the value of remaining unemployed also decreases. This drop causes the reservation wage to fall and search intensity to increase as an individual gets closer to when benefits lapse. These changes in behavior imply that the escape rate from unemployment rises until the date of benefit exhaustion. After exhaustion, the hazard rate is constant given the assumption that the environment is stationary. The time pattern of the hazard rate for an unemployed worker initially qualified for UI benefits with potential duration of P_0 periods is illustrated by the solid line in Figure 1A.[6] If individuals can locate jobs and arrange not to begin work until their benefits run out, one might observe a discrete increase in the escape rate near the point of benefits exhaustion followed by a discrete drop after exhaustion.

Mortensen's model suggests that changes in the level and length

[5] See Burdett (1979) for an analysis of a similar model.

[6] The figure is drawn assuming the marginal utility of leisure is independent of income.

of benefits have two opposing influences on the escape rate from unemployment. Increases in either of the benefit parameters have the standard disincentive effect of raising the value of being unemployed, but these increases also raise the value of being employed by increasing the utility associated with being laid off in the future. The second effect, known as the "entitlement" effect, raises the escape rate from unemployment for workers who currently do not qualify for benefits and for qualified workers close to exhaustion.

The effect of an increase in the potential duration of benefits from P_0 to P_1 is illustrated in Figure 1A. The standard disincentive effect reduces the escape rate from unemployment for a newly laid-off worker, but the entitlement effect leads to a higher escape rate as one approaches and passes the exhaustion point. The impact of an increase in the benefit level from b_0 to b_1 is illustrated in Figure 1B.

The model suggests the following stylized, reduced-form specification for the escape rate from unemployment, λ:

$$\lambda = \lambda(\overset{+}{P},\ \overset{-}{P-t},\ \overset{+}{b},\ \overset{-}{b*(P-t)},\ X) \quad \text{for } P-t \geq 0,$$

where t is the duration of the spell, P is potential duration of benefits, P-t is time until exhaustion, b is the level of benefits, and X is a vector of individual and labor market variables affecting the arrival rate of job offers, search intensity, and choice of reservation wage. The escape rate from unemployment increases as

Figure 1A: The Relation of the Escape Rate and Potential Benefit Duration

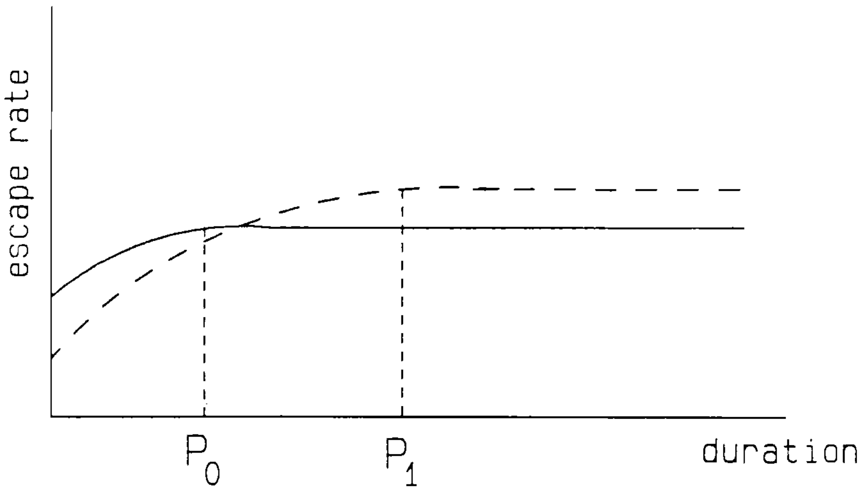
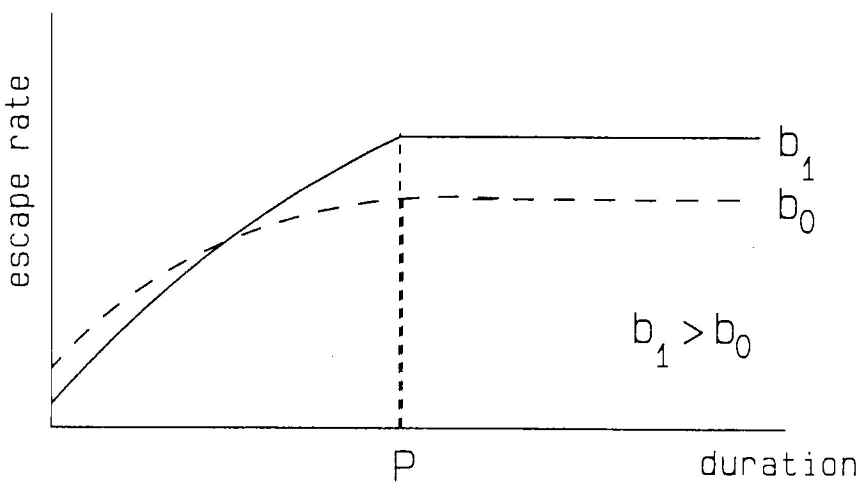


Figure 1B: The Relation of the Escape Rate and the Level of Benefits



time until exhaustion declines.[7] Higher benefits reduce the escape rate when time until exhaustion is high and increase the escape rate at around exhaustion. Since the entitlement effect is likely to be small relative to the standard search subsidy effect, the average duration unemployment is likely to rise with increases in both the level and potential duration of benefits.

Recall Prospects, UI Benefits, and Unemployment Spell Duration

The standard job search model is not entirely appropriate for analyzing the unemployment durations of workers on layoff with some possibility of recall. The interpretation of empirical evidence on the duration of insured unemployment spells in the U.S.[8] requires a consideration of the role played by recalls since the majority of insured unemployment spells appear to end in recall (Katz, 1986a; Katz and Meyer, 1988). The prospect of recall affects the probability of leaving unemployment directly through the rate of actual recalls and indirectly by affecting worker search behavior. Katz (1986a) extends a standard model of job search to include an exogenous probability of recall.[9] He shows that under reasonable

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[7]There are many reasons unrelated to UI that might generate a direct effect of spell duration on the hazard rate. Empirically, one cannot separate out the independent effects of duration, time until exhaustion, and potential benefit duration for a sample containing only insured unemployment spells if P is fixed during all spells since t, P-t, and P are perfectly collinear.

[8]A majority of insured unemployment spells in Canada also end in recall by the previous employer. See Robertson (1988).

[9]Burdett and Mortensen (1978) and Pissarides (1982) also analyze job search models that incorporate the possibility of recalls.

conditions better recall prospects reduce the new job finding rate by raising the reservation wage and reducing the likelihood of search.

The statistical model of unemployment spell durations generated by the job search models extended to allow for recalls is a competing risks model in which unemployment spells can end either through recall or the finding of an acceptable new job. The predictions of standard job search models for how variables affect the escape rate from unemployment really refer to the new job finding rate and these predictions need not hold for the overall escape rate from unemployment (the sum of the recall and new job finding rates). Information on whether spells end through recall or the finding of a new job allows an econometrician to estimate a competing risks model. Katz and Meyer (1988) have analyzed the unemployment spell durations of a sample of UI recipients in Missouri using a competing risks approach. They find that UI recipients who expect to be recalled have much lower new job finding rates than those who do not expect to be recalled, and that the new job and recall rates have quite different time patterns and are often correlated in opposite directions with characteristics of individuals.

Mortensen (1987) analyzes the effects of limited duration UI benefits in a joint wealth maximizing model of job separations that incorporates the possibility of temporary layoffs. Layoffs occur in response to reductions in match-specific productivity. The reservation wage decreases over the course of an unemployment spell as a worker approaches benefit exhaustion. This induces an increasing new job finding rate as in Mortensen (1977) and an increasing recall rate as well. Mortensen shows that for realistic parameter values most of the decline in the reservation wage should occur in the last week or two before exhaustion. The discrete change in the flow value of being unemployed when benefits are exhausted yields the prediction that many firms may recall laid-off workers around the benefit exhaustion point and that the new job finding rate should increase around exhaustion. The duration and incidence of unemployment spells are shown to rise with increases in the level and length of benefits.

Unemployment Spell Duration in a Static Labor-Leisure Choice Model

An alternative approach is taken by Moffitt and Nicholson (1982) who use a static model where unemployed workers have preferences over income and unemployment. Unemployment is valued because of its leisure component and because one can search while unemployed. At the time of job loss, individuals choose income and weeks of unemployment subject to a budget constraint. The budget constraint has a convex kink at the week of UI exhaustion because unemployment ceases to be subsidized at this point. This kink combined with a continuous distribution of tastes implies that many people will maximize their utility by returning to work the week benefits lapse. Randomness in the job finding process suggests a cluster of spells ending around the exhaustion point.

Moffitt and Nicholson show that increases in the level and length of benefits generate income and substitution effects which serve to increase the mean duration of unemployment spells. The static nature of the model makes it difficult to translate its predictions into a hazard model framework. The key prediction is that the escape rate from unemployment should be relatively high near the exhaustion point. The impacts of increases in the level or length of benefits on the choice of spell duration roughly translate into negative effects on the hazard rate.

Summary

All three models considered suggest that the distribution of unemployment spell durations should differ for UI recipients and nonrecipients. UI recipients should display stronger positive duration dependence in the new job finding rate than nonrecipients at least up through the point of exhaustion. One may also expect to find spikes in the escape rate (both through recalls and the finding of new jobs) near exhaustion for UI recipients. Increases in the level and potential duration of benefits should increase the duration of unemployment spells of UI recipients.

III. UI, Recalls, and Unemployment Spells: Evidence from the PSID

Data Description

In this section, we compare the distributions of unemployment spell durations of UI recipients and nonrecipients in the United States. We analyze employer-initiated unemployment spells in the 1980-81 period for a national sample of household heads. The data are derived from Waves 14 and 15 of the Panel Study of Income Dynamics (PSID). The interviews from these two waves of the PSID provide detailed information on each household head's last unemployment spell at least partially contained in the calendar year preceding the interview date.[10] For the last unemployment spell in the calendar year prior to the interview, respondents provide retrospective information on the spell duration and the start month of the spell.[11] We can also determine whether UI benefits were received during the spell, and whether completed spells ended through recall or the taking of a new job.

The basic sample contains 1115 layoff and plant-closing unemployment spells for household heads at least 20 and not greater than 65 years old.[12] This data set has two major advantages. First,

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[10] The questions concerning individual unemployment spells were only asked of household heads who were labor force participants at the time of the interview. Individual unemployment spell information is not available for other household members.

[11] The PSID allows one to distinguish among job separations arising from quits, plant closings, and other employer-initiated separations (layoffs and firings). The PSID does not distinguish between separations initiated through layoffs and through firings (discharges for cause).

[12] A household head's last spell from the calendar year prior to the interview date made it into the sample if (1) the spell ended in recall to the pre-separation employer; (2) the spell ended in the taking of a new job and the head was separated from his or her last job by a plant closing, layoff, or firing; (3) the spell is censored at the interview date and the head is categorized as on temporary layoff; or (4) the spell is censored at the interview date and the head is categorized as unemployed having been separated from last job by plant closing, layoff or firing. Observations satisfying the above criteria were deleted from the sample if they had missing information on pre-separation industry, occupation, spell duration, spell start date, or UI receipt. An observation was also deleted if the head at the time of the interview was not the head in the previous year.

it contains a large sample of spells for both UI recipients and nonrecipients, and contains information on complete unemployment spells rather than just compensated unemployment. UI benefits were received during some part of the spell for 63 percent of the observations (703 spells). Most studies of the impact of unemployment insurance utilize data sets containing only UI recipients and only information on weeks of compensated unemployment.[13] Second, our data set allows us to separate the escape rate from unemployment into its component parts: the new job finding rate and the recall rate. The ability to separate new job acceptances from recalls is important since the impacts of UI on job search behavior predicted by standard search models refer to the new job finding rate and do not necessarily translate into predictions for the total escape rate in samples where recalls are important.

The PSID data set also has several disadvantages. First, information is only available on whether UI is received at sometime during a spell. One cannot identify the level or potential duration of the benefits available. Second, response biases for retrospective information on individual unemployment spells can be severe. Mathiowetz and Duncan (1988) find in a study of response bias in retrospective answers to unemployment questions similar to those found in the PSID that short spells tend to be underreported and that the reported start and end dates of spells are often quite inaccurate. It is unclear whether this type of response bias distorts comparisons of the distribution of spell durations for UI

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[13] Ehrenberg and Oaxaca (1976) is an important exception.

recipients and nonrecipients. Third, the sampling frame selects the last unemployment spell at least partially contained in a calendar year and does not generate a random sample of spells. Katz (1986a) analyzes the biases introduced by this sampling frame and determines that they are unlikely to be substantial.[14] While this data set is far from perfect, it does provide a rare opportunity to look at whether the time patterns of the overall escape rate, the recall rate, and new job finding rate differ for insured and uninsured unemployment spells. The impact of variation in UI system parameters on the total escape rate from unemployment is analyzed using high quality administrative data on compensated unemployment spells in section IV.

Basic descriptive statistics for the entire sample, UI recipients, and UI nonrecipients are presented in Table 1.[15] The importance of recall for job losers in the U.S. is highlighted by the finding that 52 percent of the spells end in recall.[16] The recall rate is 64 percent for manufacturing workers, 59 percent for

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[14] Furthermore, the analysis presented in this section has been repeated on a sub-sample containing only spells that began in the fourth quarter of 1980 or the fourth quarter of 1981. This sampling frame comes close to approximating a random sample of the inflow of the relevant population into unemployment since few workers experience multiple spells in a quarter. The qualitative results are quite similar when this alternative sampling scheme is used.

[15] The PSID oversamples low income households. The extremely high nonwhite proportion of nonwhites in the sample results from this sampling scheme. The empirical findings are qualitatively quite similar to those presented in this section when only observations from the "random" (original SRC cross-section) sample are used.

[16] It is likely that this is an underestimate of the recall rate for this sample since some of the spells censored at the interview date probably ended in recall.

Table 1: Descriptive Statistics for PSID Unemployment Spell Sample

UI Recipients and Nonrecipients
Unemployment Spells Initiated by Plant Closings, Layoffs, and Firings

Variable	Description	Mean (S.D.) UI=0	UI=1	Total
Duration	unemployment spell duration in weeks	18.64	15.60	16.72
Recall	= 1 if spell ended in recall	.48	.55	.52
New Job	= 1 if spell ended in taking a new job	.28	.28	.28
Censored	= 1 if spell is censored at interview date	.24	.18	.20
UI	= 1 if received UI during some part of spell	.00	1.00	.63
Unemp. rate	= county unemployment rate	7.09	8.12	7.74
PC	= 1 if spell initiated by plant closing	.12	.07	.09
Wage	Average hourly earnings in calendar year prior to interview	5.69	7.95	7.12
Age	age in years	32.44 (10.48)	33.90 (13.59)	33.34 (10.70)
Nonwhite	= 1 if nonwhite	.61	.42	.49
Female	= 1 if female	.19	.14	.16
Married	= 1 if married	.52	.71	.64
Education	years of schooling	11.31 (2.36)	11.42 (2.14)	11.38 (2.22)
Mining	= 1 if in mining or agriculture	.04	.03	.03
Construct	= 1 if construction	.21	.17	.18
Durables	= 1 if durable goods manufacturing	.16	.36	.29
Nondurables	= 1 if nondurable goods manufacturing	.09	.15	.13
Transport	= 1 if transportation or utilities	.10	.07	.08
Trade	= 1 if wholesale or retail trade	.16	.08	.11
Service	= 1 if services	.24	.14	.18
White Collar	= 1 if managerial, professional, clerical or sales worker	.38	.22	.28
Sample size		412	703	1115

construction workers, 43 percent for transportation workers, 35 percent for service workers, and 29 percent for trade workers.

There are sharp differences in the characteristics of UI recipients and nonrecipients. UI recipients have much higher wages than nonrecipients. Substantially larger fractions of the UI recipients are white, married, male, and manufacturing workers. The recall rate is also substantially higher for UI recipients. These differences in the characteristics of the two groups help explain the longer mean spell duration for nonrecipients.[17]

Sample Hazard Functions for UI Recipients and Nonrecipients

The pattern of unemployment spell durations for UI recipients and nonrecipients from the PSID sample is illustrated in Figures 2A and 2B. The figures plot the Kaplan-Meier empirical hazards for the two samples with the weekly duration data grouped into two week intervals for ease of presentation. The overall empirical hazard for a given two week period is the fraction of spells ongoing at the start of the period which end during the two week interval. The recall and new job empirical hazards, plotted for the two samples in Figures 3A and 3B, are analogously defined as the fraction of spells ongoing at the start of a period which end during the period through recall and

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[17] Formal duration model estimates reported in Katz (1986a) that include demographic variables, industry, occupation, the county unemployment rate, and a plant closing dummy indicate that the differences in the spell durations for the two groups are largely explained by differences in these observed variables.

FIGURE2A: TOTAL HAZARD

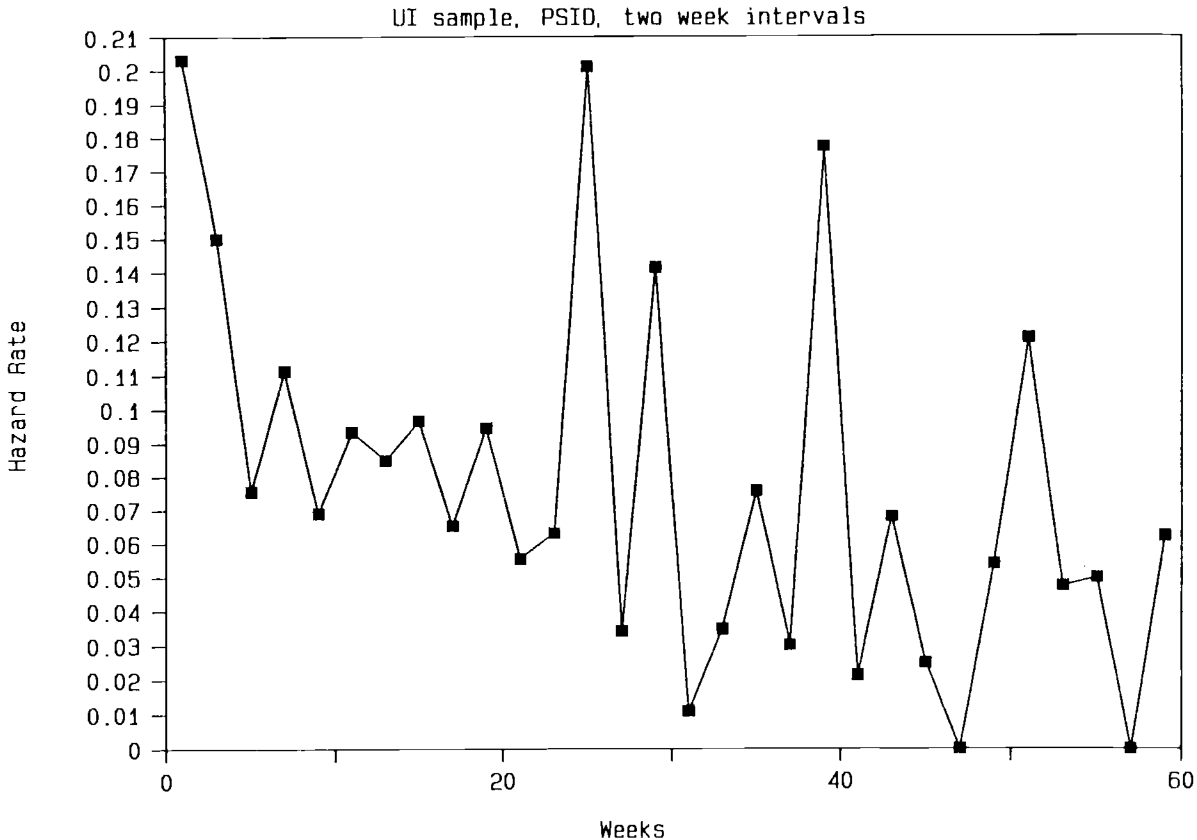


FIGURE 2B: TOTAL HAZARD

Non UI sample, PSID, two week intervals

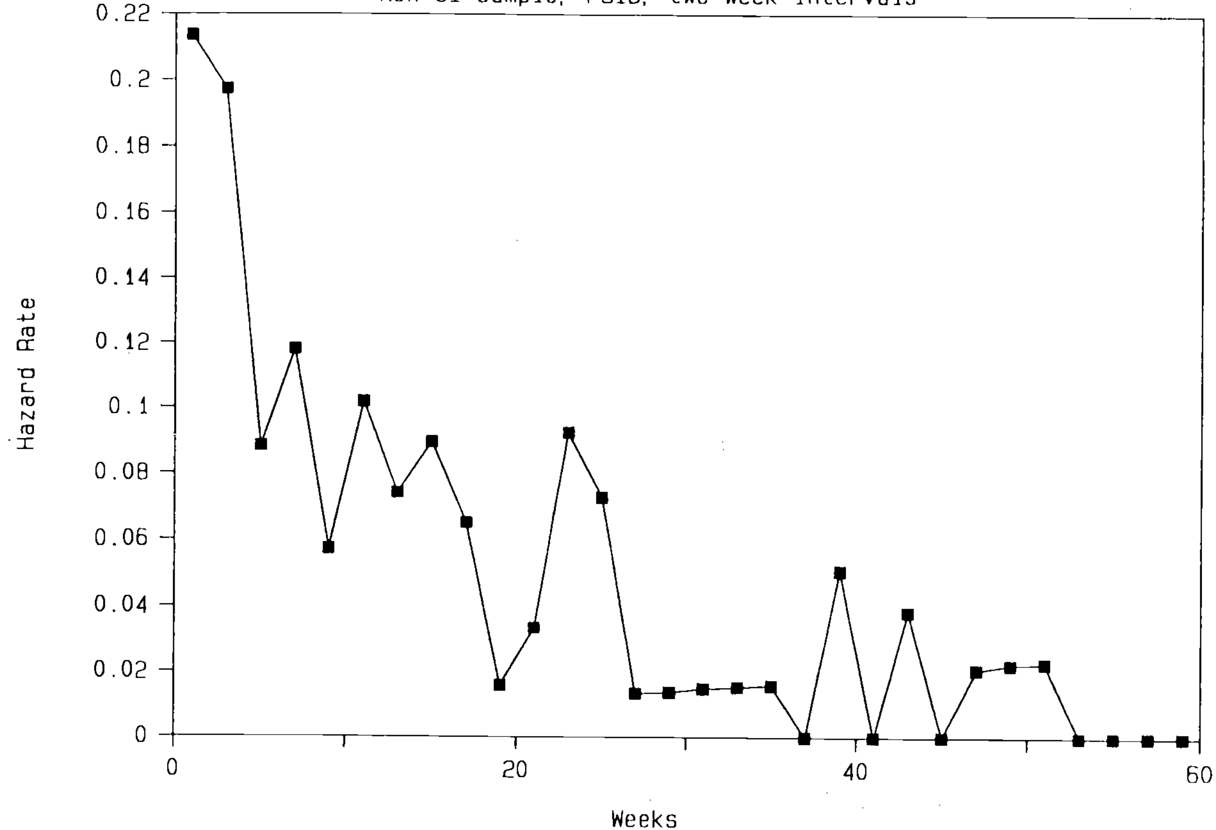


FIGURE 3A: RECALL AND NEW JOB HAZARDS

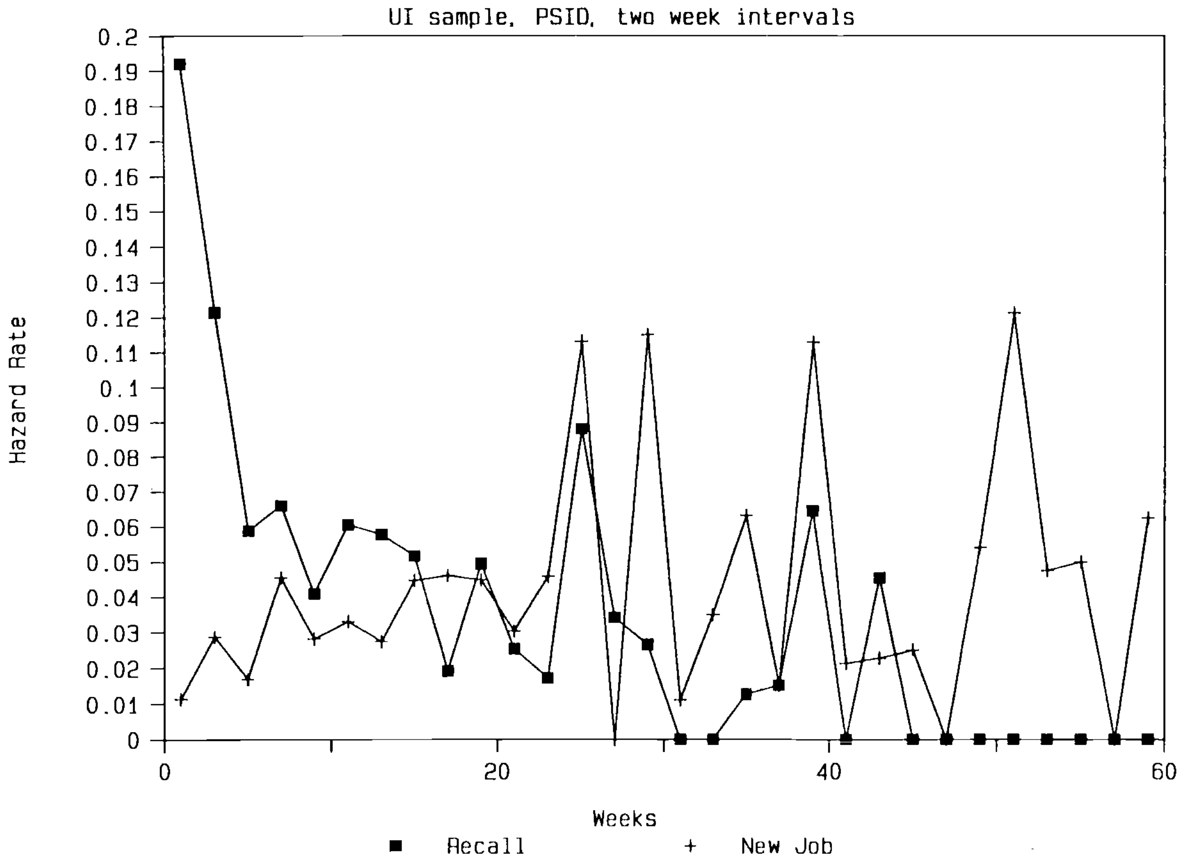
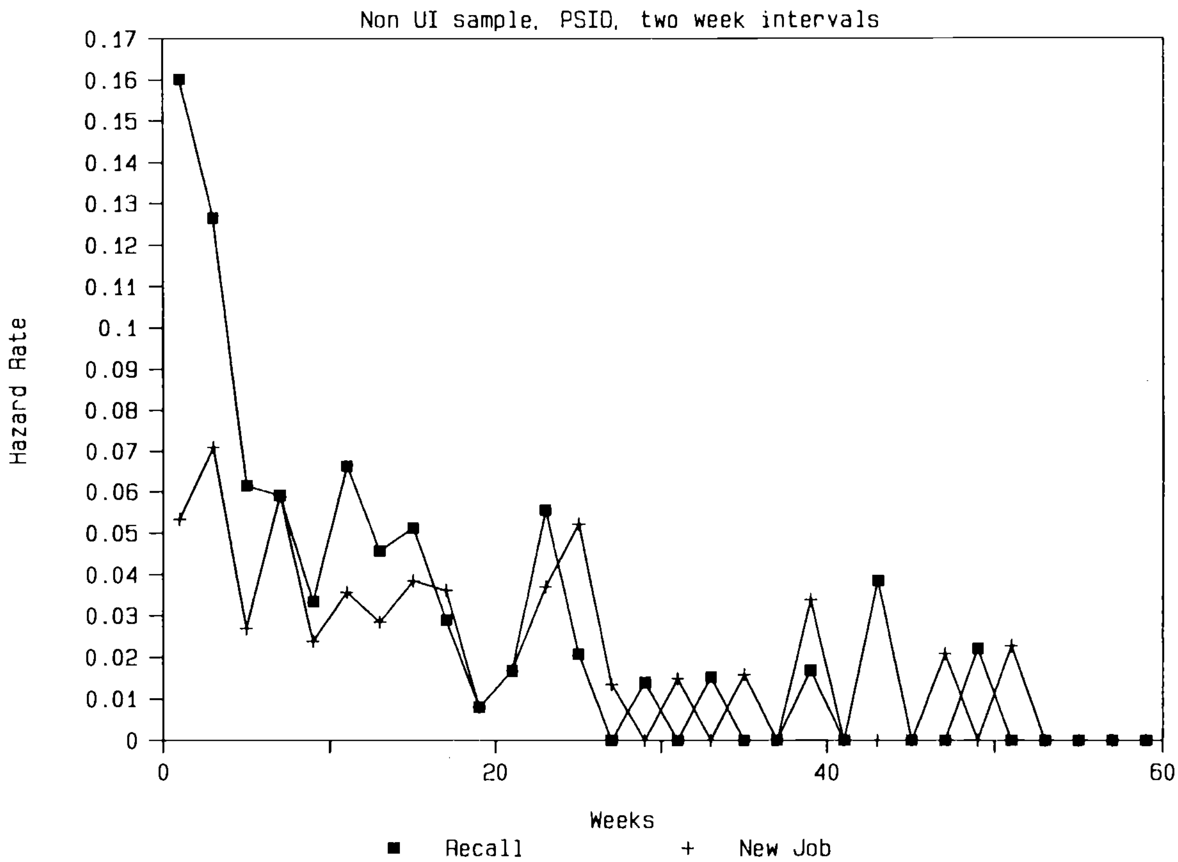


FIGURE 3B: RECALL AND NEW JOB HAZARDS



through the finding of a new job respectively.[18]

The figures reveal substantial differences in the pattern of the escape rate from unemployment for UI recipients and nonrecipients. The total hazard rates are initially downward sloping for both groups. The total hazard increases substantially in the 25 to 40 week interval for UI recipients. There are large spikes in the escape rate from unemployment at 26 weeks and at 39 weeks for UI recipients. Spikes of similar magnitude at 26 and 39 weeks are not apparent for UI nonrecipients. While the exact placing of the spikes may be an artifact of the tendency for individuals to report long unemployment rates as lasting exactly half a year, three quarters, or one year (Sider, 1985), the much greater importance of these spikes for UI recipients strongly suggests that they may be related to the limited duration of UI benefits. Most UI recipients during this period were eligible for either 26 or 39 weeks of benefits in a benefit year. The escape rate from unemployment appears to increase substantially around when many UI recipients would be exhausting benefits and a much smaller increase in the escape rate is apparent for nonrecipients.

The total hazard rates presented in Figures 2A and 2B mask sharp differences in the new job finding and recall rates illustrated in Figures 3A and 3B. The recall hazard drops sharply with spell duration for both UI recipients and nonrecipients. Most recalls occur within 8 weeks of the start date of a spell. The new job

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[18] The data behind the plots are presented in Appendix Tables A1 and A2.

finding rate differs substantially for UI recipients and nonrecipients. The new job finding rate starts out quite a bit lower and is much more upward sloping for UI recipients. The lower initial new job finding rate for UI recipients may be an artifact of the one week waiting period before UI eligibility in most states. Individuals expecting quite short spells may also not bother to apply for benefits. On the other hand, these factors could not plausibly account for the differences in the new job finding rate patterns for UI recipients and nonrecipients after the first few weeks. The low initial new job finding rate and apparent positive duration dependence in the new job finding rate for UI recipients provides support for the prediction that UI depresses new job finding when time until exhaustion is large and that the escape rate rises with time until exhaustion. The jumps in the recall and new job finding rates for UI recipients at likely exhaustion points (26 and 39 weeks) are strong evidence for the prediction that firms take into account the duration of UI benefits in designing recall policies (as suggested by the Mortensen's (1987) model of the impact of limited duration UI on recall policies) and that workers become much more likely to take new jobs as their benefits run out. The absence of such patterns for nonrecipients in the PSID sample strongly suggests that these patterns represent behavioral responses by firms and workers to the incentives created by a UI system with limited benefit duration.

The sample hazard functions plotted in the figures do not take into account heterogeneity among individuals in the sample. Although uncontrolled heterogeneity biases estimates of duration dependence in the total hazard towards spurious findings of negative duration dependence, a bias in the opposite direction is possible for an individual escape route hazard in a competing risks model. If uncontrolled factors that raise the recall rate also reduce the new job finding rate, then one can (at least in theory) generate spurious positive duration dependence in the new job hazard.[19] In fact, Katz (1986a) finds that positive duration dependence in the new job finding rate for UI recipients is more prevalent when controls for observables are included in formal duration model estimates using this PSID data set. The differences between the escape rates for UI recipients and nonrecipients and the spikes near exhaustion points for UI recipients also remain when we estimate competing risks models with nonparametric baseline hazards and controls for observables are estimated.[20] Furthermore, Katz and Meyer (1988) find substantial increases in both the recall and the new job finding rates near the week of benefits exhaustion and find strong positive duration dependence in the new job finding rate for a sample of UI recipients

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[19] Katz (1986b) shows that the conditions required for unobserved heterogeneity to lead to a bias in the direction of positive duration dependence in the new job hazard are quite extreme.

[20] Han and Hausman (1986) have developed an estimator to handle unobserved heterogeneity that is correlated among the risks in a competing risks model. They have implemented their estimator on a sub-sample of our PSID data set that excludes spells initiated by plant closings. Their results indicate that there is essentially zero correlation among the unobservables in the new job and recall hazards and that allowing for correlated, unobserved heterogeneity does not qualitatively affect one's inferences for this data set.

in Missouri.[21]

IV. Hazard Model Estimates using the Moffitt Data Set

This section reports hazard model estimates of the effect of the level and length of UI benefits on unemployment durations. We use Continuous Wage and Benefit History (CWBH) UI administrative records on the compensated unemployment spells of a sample of 3365 males from twelve states during the period 1978-1983. The sample is drawn from a data set previously analyzed by Moffitt (1985a).[22] CWBH data provide accurate information for each individual on the level of UI benefits and their potential duration. The number of weeks of benefit receipt is also known exactly.[23] This avoids many of the measurement error problems common in other data sources.

The data set provides enough variation in UI system parameters within and across states and over time to get accurate estimates of the impact of the level and length of UI benefits and the time until

[21] The Missouri data set combines UI administrative records with a follow-up survey of UI recipients. The administrative records allow one to accurately date whether a spell ends in the week that benefits lapse.

[22] The original Moffitt (1985a) data set contains 4628 observations. 1,227 observations are excluded because of missing data on age, schooling, dependents or marital status. 36 observations are excluded because the recorded spell is longer than the reported potential duration of benefits.

[23] The spells in the Moffitt data are periods of benefit receipt. Spells that are interrupted by short periods when benefits are not received are concatenated. This modified spell of benefit receipt may do a better job of grouping together periods of similar behavior. See Moffitt (1985b) for more discussion.

benefit exhaustion. On the other hand, the data set only covers compensated unemployment so that one cannot use it to make inferences about what happens to individuals after benefits are exhausted. The data set also does not permit one to identify whether spells end through recall or the finding of a new job. Thus one can only analyze the overall unemployment escape rate.

The duration of unemployment spells is analyzed using formal hazard model techniques. We use a proportional hazards model estimator that allows for time-varying explanatory variables and which nonparametrically estimates the change in the hazard over time. This semiparametric approach is analyzed in detail in Meyer (1986). The estimates are the parameters of a continuous time hazard model and thus retain a clear interpretation. Nonparametrically estimating the change in the hazard over time eliminates the need to impose a potentially restrictive functional form that has little theoretical justification. If an incorrect functional form were assumed, all of the parameter estimates from the model would be inconsistent. This danger is avoided by nonparametrically estimating the baseline hazard.

Formally, we parameterize the overall hazard rate of exit from unemployment for individual i at time t, $\lambda_i(t)$, using the proportional hazards form. Let T_i be the length of individual i's unemployment spell. Then the hazard at spell length t is

(1) $$\lambda_i(t) = \lim_{h \to 0^+} \frac{\text{prob}[t+h>T_i \geq t \mid T_i \geq t]}{h}$$

$$= \lambda_o(t)\exp\{z_i(t)'\beta\},$$

where

$\lambda_o(t)$ is the baseline hazard at time t, which is unknown,

$z_i(t)$ is a vector of time dependent explanatory variables for individual i, and

β is a vector of parameters which is unknown.

Our approach estimates β and the baseline hazard parameters $\gamma(t)$ using maximum likelihood techniques, where

$$(2) \qquad \gamma(t) = \ln\left(\int_t^{t+1} \lambda_o(u)du \right).$$

The effects of unemployment insurance are measured using functions of the benefit level and the length of benefits. The level of benefits and pre-UI earnings after state and federal taxes are used in the specifications below. Similar results are obtained when the log of benefits and earnings are used. We measure the effect of an individual's remaining potential duration of unemployment benefits on the hazard rate using the variables UI 1 to UI 41-54 which form a spline in the time until benefit exhaustion. The coefficient on UI 2-5 is the additional effect on the hazard of having moved 1 week closer to exhaustion when one is 2-5 weeks away. The coefficient on UI 1 is the additional effect on the hazard when one moves from 2 to 1 week from exhaustion. Thus, the effect of moving from 6 weeks away to 1 week is 4 times the UI 2-5 coefficient plus the UI 1 coefficient. The other UI spline coefficients have analogous interpretations.

Formally, let τ be the number of weeks until benefits lapse. Then

UI 1 = 1 if $\tau = 1$, and
0 otherwise

UI 2-5 = $\min(6-\tau,4)$ if $\tau \leq 5$, and
0 otherwise

UI 6-10 = $\min(11-\tau,5)$ if $\tau \leq 10$, and
0 otherwise,

and similarly for the remaining spline variables.

In some of the later specifications the potential duration of benefits is directly included as an explanatory variable along with the time until exhaustion spline.[24] In addition, interaction terms suggested by the theoretical models reviewed in section II and previous empirical work are included. These variables interact the level of benefits with age, the unemployment rate, and the time until exhaustion. Since benefits are extended in the course of many spells, a variable is included which equals 1 in week t if anytime during a spell it was expected that benefits would lapse in week t. This variable may also pick up the possibility that some individuals do not apply for extended benefits even when eligible. The state unemployment rate variable used in the specifications changes during each spell. State dummy variables (fixed-effects) are included in the specifications to account for unobserved differences across states that may be correlated with both the generosity of the state UI system and the character of unemployment in the state. The other included variables are age, race, education, marital status and the

[24] It is possible to allow the hazard rate to depend on time (t), time until exhaustion (P_t - t), and potential benefit duration (P_t) because P_t is a time-varying covariate in this sample. P_t changes over the course of many of the spells as extended or supplemental benefits begin or expire.

number of dependents.

Discussion of the Estimates

The coefficient estimates from the specifications are reported in Table 2. Meyer (1988) provides a detailed discussion of the estimates from similar specifications using this data set. A discussion of the non-UI variable coefficients can also be found there. In all of the specifications tried the UI benefit level has a large negative effect on the hazard rate. Specification 1 indicates that a ten percent increase in the benefit level is associated with a 5.4 percent decrease in the hazard. A strong effect of weeks until benefits lapse is seen from the exhaustion spline coefficient estimates in Specification 1. The hazard increases 94 percent when one moves from 6 weeks to 2 weeks before benefits expire. In the last week the hazard increases an additional 78 percent. Cumulatively, the hazard more than triples as one moves from 6 weeks to 1 week before exhaustion.

Since the data set we utilize contains information on weeks of benefit receipt rather than on actual weeks of unemployment, an institutional feature of state UI systems could be responsible for some of the rise in the hazard found in the last week before benefits lapse. In most states there is a cap on the total dollar amount of benefits paid to an individual during a benefit year. The cap typically depends on an individual's base period earnings. If this cap on total benefit payments is binding, an individual may receive a smaller benefit payment in his or her last week of eligibility. Some

Table 2: Semiparametric Hazard Model Estimates[a]
Moffitt Data Set (n=3365)

Variable	Mean (std deviation)	Specification (1)	Specification (2)
UI benefit level	104.23	-.0053	-.0040
(1977 dollars)	(27.91)	(.0014)	(.0015)
Pre-UI weekly earnings	169.51	.0026	.0025
after taxes (1977 dollars)	(66.52)	(.0005)	(.0005)
State unemployment rate	8.70	.0006	.0006
	(2.08)	(.0002)	(.0002)
Age 17-24	.16	.234	.688
		(.086)	(.201)
Age 25-34	.34	.117	.118
		(.076)	(.076)
Age 35-44	.24	.112	.109
		(.079)	(.079)
Age 45-54	.14	.034	.032
		(.083)	(.083)
Exhaustion spline:			
UI 1		.577	.551
		(.249)	(.250)
UI 2-5		.166	.036
		(.062)	(.099)
UI 6-10		.005	-.011
		(.032)	(.037)
UI 11-25		-.006	-.031
		(.007)	(.015)
UI 26-40		.006	-.019
		(.007)	(.019)
UI 41-54		.021	
		(.138)	
Benefits previously expected to lapse		1.537	1.578
		(.188)	(.189)
Potential duration of benefits		--	-.0247
			(.0153)
Interaction of benefit level and Age 17-24		--	-.0048
			(.0019)
Interaction of benefit level and ≤ 3 weeks until exhaustion		--	.0039
			(.0027)
Log Likelihood value		-8905.1	-8900.6

[a]Variables for education, race, marital status, number of dependents, and 11 state dummy variables are also included. The UI benefit level and pre-UI weekly earnings variables are in 1977 dollars. The numbers in parentheses are asymptotic standard errors.

individuals may not bother to pick up this smaller final check. Individuals who remained unemployed but did not collect their final UI benefit would be treated in our hazard model estimates as escaping from unemployment in the week before benefits are exhausted.

The possibility that these smaller final payments could spuriously generate the rise in the hazard just before exhaustion was examined using an additional data set. The data set contains CWBH information on 38,472 unemployment spells from 8 states during 1979-1984. The eight states include seven of the twelve in our subset of the CWBH data set previously analyzed by Moffitt. We compared the hazard rate of exit from compensated unemployment in the week before regular benefits lapse when the benefit payment was its full amount and when it was reduced because of the cap on total dollar benefits. Those who received Extended Benefits or Federal Supplemental Compensation benefits were excluded. For the eight states the hazard was 21 percent higher (25.7 percent as compared to 21.2 percent) when the last payment was less than the full weekly amount. While these comparisons are somewhat crude since we did not control for other individual attributes, they support the hypothesis that a lower benefit amount may cause people not to claim their last week of benefits. However, because only a slim majority of the final payments are less than the full amount, this effect could only cause a 12 percent increase in the overall hazard the week before benefits lapse. Thus, only a small part of the rise in the hazard just before exhaustion could be explained by this phenomenon.

The estimates reported in Table 2 also indicate that the probability of a spell ending is very high in a week in which benefits were scheduled to lapse at some point earlier in a spell. One interpretation of this result is that some firms' plan the timing of recalls and some workers' arrange the starting of a new job to coincide with the end of eligibility for benefits, but do not alter these plans in the face of an extension of benefits. Alternatively, this result could reflect that some people eligible for extended benefits do not claim them.

The estimated effect of the benefit level on the length of unemployment spells is at the high end of the distribution of recent estimates. A consensus of the previous estimates of the effect of a ten percentage point increase in the replacement ratio might be a one-half to one week increase in the length of spells.[25] Here the estimate is around one and one-half weeks. Larger estimated effects are a plausible result of better data on spell length and the level and length of benefits. Many other studies have had to impute the level of benefits for individuals and often it is not known who is even eligible for benefits.

The sources of variation in benefit levels in our data are nonlinearities in the benefit schedules (different minima and maxima across states), legislative changes during the sample period, and the erosion of real benefit levels due to inflation between legislative changes. Benefit maxima differ substantially across states. For example, the maximum benefit in Missouri is below the mean benefit in

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[25] See Hamermesh (1977) and Burtless (1986) for surveys of estimates based on U.S. data.

Pennsylvania in our data set. It is hard to make a plausible case for endogeneity of these sources of variation given that we are controlling for the previous wage, as well as state characteristics through the fixed effects. As a check on the specification however, future work is planned which will concentrate on variation in benefits due only to legislative changes.

Specification (2) in Table 2 includes several additional variables. The potential duration of benefits is included as in the original Moffitt (1985a) paper. This variable is time-varying and often increases from 26 to 39 in the course of a spell as benefits are extended. The entitlement effect captured by Mortensen's (1977) job search model leads to the prediction that the coefficient on potential benefit duration should be positive when time until exhaustion is also included as a covariate. In fact, the coefficient estimate is negative and substantial in magnitude, although it is not quite significant at conventional levels. A negative coefficient is consistent with the income effects from more generous benefits postulated by Moffitt and Nicholson (1982). The coefficient estimate implies that a 13 week extension of benefits is associated with a 27 percent decline in the hazard.

Two benefit level interaction variables are also included in Specification (2). Benefits are interacted with the dummy variable Age 17-24. This variable has a large and significant negative coefficient, indicating that the response of younger people to the benefit level is much more elastic. A larger elasticity for younger people was previously found in England by Narendranathan et al.

(1985). In a specification not reported, we also interacted the benefit level with Age 25-35. This variable had a negative coefficient, but was small and insignificant.

An interaction between the level of benefits and time until exhaustion less than or equal to 3 weeks was also added. This coefficient tends to support the hypothesis of Mortensen (1977) that higher benefits will have less of an effect near exhaustion (and may even raise the hazard), but the positive coefficient is not quite significant.

Several other specifications were tried, but are not reported. If Specification (2) is estimated without state fixed effects, the benefit level coefficient almost doubles in absolute value to .0073 (standard error .0011), while the estimated time until exhaustion and potential benefit duration effects are not greatly altered. The exclusion of fixed effects also causes the state unemployment rate to change sign while retaining statistical significance. It appears that higher unemployment states also tend to have longer spells, but when the unemployment rate rises within a given state the mean spell length drops. This finding may be the result of a composition effect arising from the greater frequency of short temporary layoffs in downturns. Other interactions between the benefit level and time until exhaustion were tried, but they were small and insignificant.

Further specifications were estimated which explicitly accounted for the potential impact of omitted variables on the hazard model estimates. These left out variables are typically called unobserved heterogeneity. Specifications which allowed unobserved heterogeneity with a gamma distribution were tried. The signs and statistical significance of the main coefficients did not change appreciably, but the benefit coefficients (benefit level, benefit and age interaction, potential duration of benefits) rose by about 25 percent.

V. Simulating the Impact of Changes in the Length and Level of UI Benefits

In this section, we simulate the effect of changes in the level and length of UI benefits on the duration of unemployment spells, the exhaustion rate, and the amount of benefits paid. Simulations are useful to better describe the effects of changes in covariates because the transformation from a change in the hazard to changes in spell length is complex. The complexity arises from the nonlinearity of the model and the estimation of a separate baseline hazard parameter for each spell length interval. Simulations also provide a degree of robustness to unobserved heterogeneity. One interpretation of results presented in Lancaster (1985) is that unobserved heterogeneity biases coefficient estimates toward zero but will have little effect on elasticities of mean duration with respect to covariates.[26] Our simulations essentially calculate these mean duration elasticities. The simulations also allow one to use the entire distribution of explanatory variables rather than evaluating the model at the mean of the covariates. Finally, changes in the

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[26] Strictly speaking, Lancaster's result only applies to an uncensored Weibull model without time-varying explanatory variables.

amount of benefits paid and the exhaustion rate are not easily calculated without simulations.

The simulations are somewhat speculative since they only use information on the unemployment spell durations of UI recipients and thereby do not illuminate how changes in UI parameters may affect wages, the incidence of layoff unemployment or the unemployment experience of non-UI recipients through possible displacement effects.[27]

Our simulations use the actual sample distribution of the time-invariant covariates from our subsample of the Moffitt data. The parameter estimates used in the simulations are those in Table 2. The simulations assume that state unemployment rate is constant during each spell and equal to the start of spell value for each individual in the sample. It is assumed that everyone is eligible for 39 weeks of benefits (the standard potential duration in a period in which extended benefits are triggered). The most speculative part of the procedure is making an assumption concerning behavior after benefits are exhausted. Since the data set covers only compensated unemployment, one cannot use it for inferences concerning post-exhaustion escape rates. We assume that after exhaustion the baseline hazard is equal to the average baseline hazard is our sample and that the benefit level is zero. The exhaustion spline is treated as if one is 15 weeks before exhaustion in the simulations presented

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[27]See Burtless (1987) and Narendranathan et al. (1985) for a discussion of the difficulties in going from micro estimates of the effect of UI parameters on unemployment duration to conclusions concerning aggregate unemployment.

here. The rationale is to avoid the high escape rate from temporary layoffs at the early part of spells and the exhaustion spike found close to the exhaustion point. Katz and Meyer (1988) find for a sample of UI recipients in Missouri that the overall hazard does decline substantially after exhaustion. This decline is largely accounted for by an extremely low recall rate after exhaustion. The simulations are not appreciably changed if the exhaustion spline is treated as if one who exhausts benefits has the exhaustion spline values of an individual at 25 weeks before exhaustion.

Simulation Methodology

The next few paragraphs formally describe the simulation methodology. The key quantity used in the simulations is the predicted survivor function for each individual in week t, conditional on the individual's covariates $z_i(\tau)$ up until t. The predicted survivor function in week t is the predicted probability of a spell lasting at least until t and it is defined by the equation

$$(3) \quad \hat{S}_i(t) = S_i(t|\hat{\gamma}(\tau), z_i(\tau)'\hat{\beta}; \tau=0, \ldots, t-1) = \exp(- \int_0^t \hat{\lambda}_i(u)du)$$

$$= \exp(-\sum_{\tau=0}^{t-1} \exp[\hat{\gamma}(\tau)+z_i(\tau)'\hat{\beta}]),$$

where a hat above an expression denotes an estimated quantity. The aggreagate survivor function for the sample is then defined by

$$(4) \quad \bar{S}(t) = \frac{1}{N} \cdot \sum_{i=1}^{N} \hat{S}_i(t) \quad ,$$

where N is the sample size. Given the aggregate survivor function, the predicted mean weeks of unemployment is calculated using the rolling sum which is the predicted weeks of unemployment accumulated by week t.

$$(5) \quad \text{Weeks accumulated by } t \equiv M(t) \equiv \sum_{\tau=1}^{t} \bar{S}(\tau) \ .$$

In all of the simulations M(104), the number of weeks accumulated by the end of two years, was calculated. Since the sum converged rapidly the simulation results would not be very different if we had truncated the sum at 1 or 3 years instead. Thus, the predicted mean weeks of unemployment is defined by M(104).

Predicted mean weeks compensated is defined by M(d), where d is the potential duration of benefits. Predicted benefits paid per spell, B(d) is defined by

$$(6) \quad B(d) \equiv \frac{1}{N} \cdot \sum_{i=1}^{N} \sum_{\tau=1}^{d} \hat{S}_i(\tau) \cdot b_i \ ,$$

where b_i is the UI benefit for individual i. Finally, the predicted percentage exhausting UI benefits equals $\bar{S}(d)$.

<u>Simulation Results</u>

Simulations which use Specification 1 are reported in Table 3, and simulations using Specification 2 are reported in Table 4. The base case scenario predicted values differ appreciably for the two simulations, but the effects of policy changes are very similar. The base case difference occurs because the simulations assume that potential duration does not change during the unemployment spells, while in the actual Moffitt data the potential duration benefits rises in the course of many spells as benefits are extended. This effect is captured through the baseline hazard estimates in Specification 1 rather than directly in the potential duration of benefits coefficient as in Specification 2. In both sets of simulations the potential duration of benefits is assumed to be constant over time, but in Specification 1 the baseline hazard estimates implicitly incorporate increases in the potential duration of benefits from extended benefits triggers turning on during the course of a spell. Thus the simulations from Specification 2 presented in Table 4 probably provide a better guide to the mean weeks of unemployment and benefit payments to be expected in each of the scenarios.

We simulated the impact of changes in UI parameters on the predicted mean completed spell of unemployment and on the mean weeks of compensated unemployment. Tables 3 and 4 report the following policy experiments: 10, 20 and 30 percent reductions in the level of benefits, and changes in the potential duration of benefits from 39 to either 35 or 26 weeks. A change in maximum potential benefit duration from 26 to 39 weeks is exactly the natural policy experiment that occurs when extended benefits are triggered in the United States.

Changes in the level of benefits and changes in the potential

Table 3: Simulations Using Specification (1) in Table 2

(The numbers in parentheses are percentage changes from the base case)

Scenario	Predicted Mean Weeks of Unemployment	Predicted Mean Weeks Compensated	Predicted Benefits Paid Per Spell	Predicted Percentage Exhausting
Base Case (39 weeks)	18.4	16.6	$ 1796	12.9
Benefit Level Reduced 10%	16.9 (-8.2)	15.5 (-6.6)	1503 (-16.3)	10.4 (-19.4)
Benefit Level Reduced 20%	15.4 (-16.3)	14.4 (-13.3)	1236 (-31.2)	8.2 (-36.4)
Benefit Level Reduced 30%	14.1 (-23.4)	13.3 (-19.9)	996 (-44.5)	6.3 (-51.2)
Potential Benefit Duration Reduced to 35 weeks	17.6 (-4.3)	15.7 (-5.4)	1690 (-5.9)	14.6 (13.2)
Potential Benefit Duration Reduced to 26 weeks	16.2 (-12.0)	13.6 (-18.0)	1461 (-18.7)	20.7 (60.5)

Table 4: Simulations Using Specification (2) in Table 2

(The numbers in parentheses are percentage changes from the base case)

Scenario	Predicted Mean Weeks of Unemployment	Predicted Mean Weeks Compensated	Predicted Benefits Paid Per Spell	Predicted Percentage Exhausting
Base Case (39 weeks)	14.7	13.5	$ 1455	9.0
Benefit Level Reduced 10%	13.5 (-8.2)	12.5 (-7.4)	1215 (-16.5)	7.2 (-20.0)
Benefit Level Reduced 20%	12.4 (-15.6)	11.6 (-14.1)	999 (-31.3)	5.7 (-36.7)
Benefit Level Reduced 30%	11.3 (-23.1)	10.8 (-20.0)	807 (-44.5)	4.4 (-51.1)
Potential Benefit Duration Reduced to 35 weeks	13.9 (-5.4)	12.8 (-5.2)	1376 (-5.4)	9.8 (8.9)
Potential Benefit Duration Reduced to 26 weeks	12.6 (-14.3)	11.4 (-15.6)	1222 (-16.0)	13.4 (51.1)

length of benefits have substantial effects on the mean duration of unemployment of UI recipients. The two sets of simulations provide quite similar estimates of the impact of policy changes. An increase in the potential duration of benefits from 26 to 39 weeks is predicted to raise the mean unemployment spell duration by 2.1 weeks in both simulations. This is a surprisingly large effect given that most spells are completed well before the 26 weeks of regular benefits run out. An increase in potential benefit duration will mechanically increase the mean compensated spell duration even if benefits have no incentive effect since previously uncompensated unemployment will be classified as compensated unemployment. The predicted effect of an extension of benefits from 26 weeks to 39 weeks if there are no incentive effects from extending benefits (using Specification 2) is a 0.9 week increase in mean compensated spell duration. Thus most of the impact of extended benefits on compensated unemployment arises through the negative effects of UI on the escape rate from unemployment.

We conclude from an examination of a variety of simulated changes in potential benefit durations that a one week extension of benefits increases the mean duration of an unemployment spell by approximately 0.16 to 0.20 weeks. One caveat in interpreting these estimates is that much of the variation in the potential length of benefits arises from the extension of benefits in times of poor macroeconomic conditions. If the time-varying state unemployment rate variable included in our specifications does not fully capture labor market conditions, then part of the our estimate of the increase in the duration of unemployment from an increase in potential benefit duration may simply reflect that potential benefit duration is high when job availability is low. However, the estimated effects of UI are corroborated by Specification 1 which does not use this source of variation in the UI parameters and still gives similar results.

Our estimates of the impact of potential benefit duration on the average unemployment spell duration of UI recipients are a bit larger than most of those in the literature. Our estimates are slightly larger than Moffitt's (1985a) estimate of 0.15 weeks from a model that does not include state dummy variables. Moffitt and Nicholson (1982) find using a static labor supply estimation framework that a one week extension raises the average unemployment duration by 0.10 week. Moffitt and Nicholson's sample includes only UI recipients who had exhausted their regular benefits, and their measure of unemployment includes both compensated and uncompensated unemployment. It seems plausible that a group of largely "long-term" unemployed workers, such as the sample analyzed by Moffitt and Nicholson, may be less sensitive to benefits than a more representative group of UI recipients such as in the data set we examine. In a study of Canadian UI recipients, Ham and Rea (1987) find that a one week increase in the duration of benefits increases the mean duration to the start of a new job by 0.26-0.33 weeks in a competing risks framework.[28]

The hypothetical UI parameter changes examined in the simulations

[28] Since some of the spells in their sample end in recall, it is difficult to translate this finding into an estimate of the effect on the mean duration of unemployment.

have substantial effects on the amount of benefits paid per spell. A reduction in the level of benefits by 10 percent has an impact on the UI budget similar to a reduction in the potential duration of benefits from 39 to 26 weeks. The simulations indicate that increases in potential benefit duration have much larger adverse incentive effects on unemployment than do changes in the level of benefits that have the same effect on the UI budget. The simulations in Table 4 show that the budget cut from the base case accomplished through a 10 percent reduction in benefits reduces mean unemployment by 1.2 weeks, while a similar budget cut done through eliminating extended benefits generates almost twice as large a reduction in unemployment.

These findings suggest that a government with a fixed UI budget faces a sharp trade-off between incentives and insurance in the design of the level and time sequence of UI payments. A balanced budget reduction in the level and increase in the maximum duration of benefits has strong adverse incentive effects although it does provide greater protection for those who are unlucky in their attempts to gain reemployment. The theoretical results of Shavell and Weiss (1979) concerning the optimal design of a UI system subject to a fixed UI budget combined with our findings of strong behavioral effects of UI on the rate of reemployment suggest that a system of high benefits with limited duration may be preferable to one with lower benefits of longer duration.

The Impacts of Extended Benefits on the Income of the Unemployed

Broadly, our results suggest that the behavioral effects of UI are extremely important. In fact, the estimated incentive effects of extended benefits are large enough to allow the possibility that benefit extensions could actually reduce the total money income of UI recipients. If the benefit extension did not affect the duration of unemployment or reemployment earnings, then increasing the weeks of unemployment in which benefits are received would unambiguously raise the income of the unemployed. On the other hand, if a higher duration of benefits increases unemployment duration and does not affect reemployment wages, the extension of benefits may reduce the income (although probably not the welfare) of UI recipients if reemployment wages are higher than UI benefits.

The following simple calculations are instructive concerning the incentive effects of increases in benefit duration. The simulations presented in Table 4 imply that an increase in benefit duration from 26 to 39 weeks raises the unemployment income of typical UI recipients by $223 (from $1222 to $1455 in 1977 dollars). If one assumes that there are no behavioral effects of extending benefits, i.e. the distribution of spells is unchanged, then the analogous figure is $98.[29] The overall impact of this increase in potential benefit duration on the income of the typical UI recipient may be assessed under alternative assumptions about reemployment wages. We make the strong assumption that reemployment weekly wages are

[29] This calculations assumes that the escape rate from unemployment after 26 weeks is the same as it would be if benefits had not been extended.

unaffected by the availability of extended benefits. Under this assumption, the change in a UI recipient's money income arising from the extension of benefits is given by the formula:

$$\Delta \text{ Income} = \{ \Delta(\text{Weeks of compensated unemployment}) * (\text{Weekly UI benefit}) \} - \{ \Delta(\text{Total Weeks of unemployment}) * (\text{Reemployment weekly wage}) \}.$$

In Table 5, we present the predicted impact on the income of a UI recipient (with pre-UI weekly earnings equal to our sample average of $170) of an increase in potential benefit duration from 26 to 39 weeks. We use the simulations discussed above based on specifications (1) and (2) from Table 2. We consider three cases for each specification. The first case assumes there are no behavioral effects of extended benefits. The second case assumes that extended benefits increase unemployment by the amounts shown in our simulation results (in Tables 3 and 4) and that reemployment wages are 90 percent of pre-UI weekly wages. The final case assumes these same behavioral effects on the duration of unemployment, but assumes that reemployment wages are only 60 percent of pre-UI weekly wages. Katz and Meyer (1988) find that the typical UI recipient who gains reemployment within a year of layoff has (initial) reemployment weekly earnings that are approximately 10 percent less than pre-UI weekly earnings. On the other hand, those who were not recalled and exhausted benefits averaged 50 percent losses in weekly earnings.

The calculations presented in Table 5 suggest that extending benefits may reduce the total money income of UI recipients. If one

Table 5: The Effect of An Increase in Potential Benefit Duration from 26 to 39 Weeks on the Income of a Typical UI Recipient

Scenario	Change in Unemployment Income	Change in Wage Income	Net Change in Income
Specification (1) from Table 2			
No behavioral effects of UI	$179	$ 0	$ 179
Behavioral Effects:			
Reemployment weekly wage equals 90% of pre-UI weekly earnings	336	-337	-1
Reemployment weekly wage equals 60% of pre-UI weekly earnings	336	-225	111
Specification (2) from Table 2			
No behavioral effects of UI	$ 98	$ 0	$ 98
Behavioral Effects:			
Reemployment weekly wage equals 90% of pre-UI weekly earnings	223	-321	-98
Reemployment weekly wage equals 60% of pre-UI weekly earnings	223	-214	9

These calculations assume a pre-UI weekly wage of $170. All figures are in 1977 dollars. The change in income of a UI recipient arising from the extension of benefits is given by the formula:

$$\Delta \text{ Income} = \{ \Delta(\text{Weeks of compensated unemployment}) * (\text{Weekly UI benefit}) \} - \{ \Delta(\text{Total Weeks of unemployment}) * (\text{Reemployment weekly wage}) \}.$$

assumes that reemployment earnings are 90 percent of previous earnings, then both specifications yield the prediction that the income of the typical UI recipient actually falls in response to an increase in potential benefit duration from 26 to 39 weeks. If workers whose behavior is most strongly affected by extended benefits have low reemployment wages, then it is likely that extended benefits raise the money income of UI recipients. Of course, these calculations ignore the increases in leisure accruing to UI recipients from greater unemployment and do not take into account the possibility that extended benefits may allow workers to make better job matches raising future earnings from employment. If a longer duration of benefits allows workers to find higher paying jobs and these jobs last for even several months, then extended benefits are likely to raise the income of the unemployed.

VI. Conclusions

The evidence presented in this paper indicates that the potential duration of UI benefits has a strong impact on the duration of the unemployment spells of UI recipients in the United States. Our examination of data from the PSID indicates that the distributions of unemployment spell durations of UI recipients and nonrecipients are quite different. Substantial increases in both the recall rate and new job finding rate are apparent for UI recipients around the time when benefits are likely to lapse. Large increases in the escape rate from unemployment in the several weeks before exhaustion are also apparent for a large sample of UI recipients for which administrative data allows us to accurately date the end of the spell and the point at which benefits are exhausted. Katz and Meyer (1988) report similar increases in the hazard rate near exhaustion for another sample of UI recipients. It seems safe to conclude that potential benefit duration has significant behavioral effects on firm recall policies and worker new job finding strategies. Furthermore, our estimates indicate that policies that extend benefits have much greater adverse incentive effects on the duration of unemployment than policies with the same predicted impact on the government budget which raise the level of benefits.

Our results indicate that a one week increase in potential benefit duration increases the average duration of the unemployment spells of UI recipients by about 0.16 to 0.20 weeks. These estimates can be used guardedly to make a rough guess as to what the impact of longer potential benefit durations in Europe than in the United States is on the mean duration of unemployment. An increase in potential benefit duration from six months to one year is predicted to increase mean duration of unemployment by 4 to 5 weeks, and an increase from six months to two years is predicted to generate a 13 to 16 week increase in unemployment duration. The average uncompleted duration of ongoing spells was 68.2 weeks in the United Kingdom in 1984 versus 18 weeks in the United States (Layard and Nickell, 1986). The fraction of the unemployed covered by benefits is also much lower in the United States than in the United Kingdom.[30] Thus, longer duration of benefits may be able to explain about 10 to 30 percent of the difference in mean unemployment spell durations between the United States and the United Kingdom.

Two caveats about our results should be kept in mind. First, while lower unemployment benefits might decrease the length of UI recipients' spells, the spells of non-UI recipients might rise due to congestion/displacement effects. If aggregate employment is determined by the level of demand, and the matching of particular workers to jobs is not important, shorter unemployment spells for one group would imply longer spells on average for others. This effect would imply that our estimates of the microeconomic effects of UI on unemployment are an overestimate of the macroeconomic effects. Second, we have concentrated on transitions in one direction between only two of the possible labor market states. Clark and Summers (1982) and Topel (1985) have emphasized the effects of UI on other transitions. A more encompassing analysis of the effects of UI on the labor market might yield different conclusions about the aggregate effects of changes in the level and length of UI benefits.

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[30] Blank and Card (1988) and Kane (1988) document the recent decline in the fraction of the unemployed receiving UI in the U.S. and examine alternative explanations for this phenomenon.

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APPENDIX

Table A1: Empirical Hazards for UI Recipients
PSID Data, 2 Week Escape Rates

		# of Escapes		Empirical Hazards		
Weeks Unemployed	Risk Set	Recall	New Job	Total	Recall	New Job
1-2	703	135	8	0.2034	0.1920	0.0114
3-4	560	68	16	0.1500	0.1214	0.0286
5-6	476	28	8	0.0756	0.0588	0.0168
7-8	440	29	20	0.1114	0.0659	0.0455
9-10	391	16	11	0.0691	0.0409	0.0281
11-12	364	22	12	0.0934	0.0604	0.0330
13-14	329	19	9	0.0851	0.0578	0.0274
15-16	290	15	13	0.0966	0.0517	0.0448
17-18	260	5	12	0.0654	0.0192	0.0462
19-20	222	11	10	0.0946	0.0495	0.0450
21-22	198	5	6	0.0556	0.0253	0.0303
23-24	174	3	8	0.0632	0.0172	0.0460
25-26	159	14	18	0.2013	0.0881	0.1132
27-28	117	4	0	0.0342	0.0342	0.0000
29-30	113	3	13	0.1416	0.0265	0.1150
31-32	91	0	1	0.0110	0.0000	0.0110
33-34	86	0	3	0.0349	0.0000	0.0349
35-36	79	1	5	0.0759	0.0127	0.0633
37-38	66	1	1	0.0303	0.0152	0.0152
39-40	62	4	7	0.1774	0.0645	0.1129
41-42	47	0	1	0.0213	0.0000	0.0213
43-44	44	2	1	0.0682	0.0455	0.0227
45-46	40	0	1	0.0250	0.0000	0.0250
47-48	39	0	0	0.0000	0.0000	0.0000
49-50	37	0	2	0.0541	0.0000	0.0541
51-52	33	0	4	0.1212	0.0000	0.1212
53-54	21	0	1	0.0476	0.0000	0.0476
55-56	20	0	1	0.0500	0.0000	0.0500
57-58	17	0	0	0.0000	0.0000	0.0000
59-60	16	0	1	0.0625	0.0000	0.0625

Table A2: Empirical Hazards for UI Nonrecipients
PSID Data, 2 Week Escape Rates

		# of Escapes		Empirical Hazards		
Weeks Unemployed	Risk Set	Recall	New Job	Total	Recall	New Job
1-2	412	66	22	0.2136	0.1602	0.0534
3-4	324	41	23	0.1975	0.1265	0.0710
5-6	260	16	7	0.0885	0.0615	0.0269
7-8	237	14	14	0.1181	0.0591	0.0591
9-10	209	7	5	0.0574	0.0335	0.0239
11-12	196	13	7	0.1020	0.0663	0.0357
13-14	175	8	5	0.0743	0.0457	0.0286
15-16	156	8	6	0.0897	0.0513	0.0385
17-18	138	4	5	0.0652	0.0290	0.0362
19-20	125	1	1	0.0160	0.0080	0.0080
21-22	119	2	2	0.0336	0.0168	0.0168
23-24	108	6	4	0.0926	0.0556	0.0370
25-26	96	2	5	0.0729	0.0208	0.0521
27-28	74	0	1	0.0135	0.0000	0.0135
29-30	72	1	0	0.0139	0.0139	0.0000
31-32	67	0	1	0.0149	0.0000	0.0149
33-34	65	1	0	0.0154	0.0154	0.0000
35-36	63	0	1	0.0159	0.0000	0.0159
37-38	61	0	0	0.0000	0.0000	0.0000
39-40	59	1	2	0.0508	0.0169	0.0339
41-42	52	0	0	0.0000	0.0000	0.0000
43-44	52	2	0	0.0385	0.0385	0.0000
45-46	48	0	0	0.0000	0.0000	0.0000
47-48	48	0	1	0.0208	0.0000	0.0208
49-50	45	1	0	0.0222	0.0222	0.0000
51-52	44	0	1	0.0227	0.0000	0.0227
53-54	32	0	0	0.0000	0.0000	0.0000
55-56	32	0	0	0.0000	0.0000	0.0000
57-58	32	0	0	0.0000	0.0000	0.0000
59-60	32	0	0	0.0000	0.0000	0.0000